This is the most difficult case. When a perfect cube ends in 25 its root must end in an even number(0 being included) followed by 5. If a perfect cube ends in 75, its root must end in an odd number followed by 5. Find the cube root of 61,629,875. The root is 315, 335, 355, 375 or 395. Dividing 61,629,875 by 6 we get 5 for a remainder. The same remainder is obtained from 335 and 395. Hence we have only to decide between 335 and 395. The 61 plainly indicates 395.

In a future article I shall discuss the relation of the hundreds of the cube to the hundreds of the root.

LETTER FROM PROF. SCHIAPARELLI.

EDITOR OF THE ANALYST, DES MOINES, IOWA:—Prof. A Hall of Washington, has had the kindness to send me No. 2 of your journal, THE ANALYST, containing an article with the title "Comets and Meteors." In this article Mr. Hall offers some objections to the difficulties which I have opposed to the calculation of Laplace on the probabilities of hyperbolic orbits, (*Conn. des Temps* for 1816, pp. 215–218). I ask of you permission to add some observations in reply to the remarks of Mr. Hall.

The first remark is thus stated: "If S be the sun and P the point on the surface of the sphere of activity of the sun's attractive force, the bodies that pass through P may have all possible directions, but a direction making a small angle with PS is less probable than one making a greater angle. I cannot see that Prof. Schiaparelli has introduced this condition into his solution." I have always been persuaded of the truth of the proposition announced by Mr. Hall. If I have not explicitely spoken of it, it is only because the course of the demonstration does not require it. My solution would have been erroneous if I had supposed the contrary proposition; that is to say, that all the angles with $P\ S$ are equally probable. Mr. Hall will have some difficulty to prove that I have committed an error of this gravity. In order to establish that a demonstration is in fault it is not sufficient to say that this or that idea is not indicated; it is necessary to find some evident error either in the fundamental supposition or in the logical connection of the reasoning.

The second remark of Mr. Hall refers to a detail of calculation; that is to say, to this, that in seeking to find the defect of the solution of Laplace I have not spoken of the division by U, indicated by this great geometer at the beginning of page 216 of his memoir. I shall observe here that U is the greatest value physically attributable to the velocity v; it is there

fore a constant limit. As it is required here not to calculate the *absolute* probabilities but only their *ratios*, it is quite indifferent whether we introduce or not the common divisor U. This is so true that when Laplace compares the two probabilities he finds for their ratio the equation, (page 218),

$$\frac{\pi-2}{100}\sqrt{\frac{r}{2D}(r+200)}-1:1,$$

from which U has completely disappeared. As in my investigation I sought the essential point, I was not occupied with this division by U, to which I have not the least objection since it is wholly foreign to our question.

My difficulties remain therefore intact; they are represented in the two following propositions:

I. The integral $\int_a^\infty dv\left[1-\frac{\sqrt{1-\frac{D}{r}}}{r\,v}\sqrt{r^2\,v^2\left(1+\frac{D}{r}\right)-2\,D}\right]$ ..(a)

is infinite whatever may be the finite value of the lower limit a.

II. By means of an incomplete development into a series Laplace has found for this quantity a finite value, which has completely changed the final result of his research.

So long as one has not demonstrated the inexactness of the one or the other of these two propositions I cannot regard as destroyed the objections that I have raised against the analysis of Laplace. I will add that it is erroneous in another place. In fact, in evaluating the probabilities Laplace excludes all the bodies for which the values of v are comprised between the limits

$$v=0,\text{ and }v=\frac{\sqrt{2D}}{r\sqrt{1+\frac{D}{r}}},\quad\ldots\ldots\ldots\ldots\ldots(b)$$

apparently for the reason that he derives an imaginary value for the second of the radicals contained in the expression (a). But this is not a sufficient reason. In fact, when v is comprised between the limits indicated, the imaginary value of (a) signifies that the perihelion distance of the orbits described by these bodies cannot be equal to D, and that it is always less than D. These bodies ought therefore to enter into the account in determining the probability; only for them it is necessary to calculate the probability in another manner.

I must add, in order to be just, a remark which escaped me in 1871, at the time of the publication of my book on the meteoric stars. I say that

these two objections that I have found to the investigation of Laplace, although theoretically established, are not of a great practical consequence for the resolution of his problem. In fact, as to that which regards the omission of the velocities comprised between the limits (b), it is easy to show that these velocities always correspond to elliptic orbits. It follows that in rectifying this omission we shall augment the number of orbits very elongated, which, according to Laplace, ought to be the most numerous; which would only reinforce his conclusion.

As to the error in the evaluation of the integral (a), it exists only when we extend the upper limit to infinity; that is to say, when we suppose equally probable all the velocities from 0 even to ∞. This supposition is physically inadmissable; it is necessary therefore to give a finite value to the upper limit of the integration. In putting for this limit a moderate value of v, as 10, 20 or 50, we find that the integral (a) does not differ much from the value assigned by Laplace when extending it even to infinity, one error corrects the other in such a way that practically his final conclusion appears to be very nearly justified.

In resuming, the inadvertencies which I have noticed only affect the mathematical perfection of the problem; but they do not really change much the practical value of the final conclusion drawn by Laplace. This is what I should have recognized sooner, viz: That the true cause of the error of this final conclusion must be sought for in the fundamental suppositions, and precisely in this fact that Laplace has neglected to consider the motion of the solar system in space. In 1813 the astronomers had not much confidence in the speculations of W. Herschel on the proper motion of the solar system; they could therefore reasonably exclude it from consideration. This is not permitted to-day. In reconsidering the problem therefore under the point of view of Laplace, but with the supposition that the solar system is transported in space with the veloclty u, we shall find not only a very great excess of probability in favor of orbits strongly hyperbolic; but we shall see moreover that the hyperbolas whose axes approach this quantity

$$-\frac{1}{u^2}$$

must be more frequent than others. This being contrary to observation, we must conclude that the comets are not bodies of a stellar nature.

Accept, Sir, the expression of my sincere respect,

J. V. Schiaparelli.

Director of the Observatory, Milan, Italy.